

April 20, 1955

Dr. Bruce Stocker
Lister Institute
London, England

Dear Bruce:

I am afraid this parcel will be something of a disappointment. As my earlier letters indicated, I had dropped everything else since the beginning of the year in hopes of clearing up the trails story at this end; it has turned out a much more formidable task than I would have anticipated. I am particularly anxious to return to the conjugal colic which I had to abandon last Xmas. Had nothing else supervened, I would have preferred to wait until I had a chance to think the matter through, but this might mean a year's additional delay and would not be fair to you, and especially to your student.

So I tried to see about amending your draft, and found this a most discouraging task. We might have managed this a year ago, and I am sorry now we did not, but since then we have not in fact been doing the same experiments. I found I could not conscientiously share the responsibility ~~and~~ or the credit for pedigrees (on the lineage of E) such as I had not duplicated, and when you mentioned the ~~desire~~ desirability of bringing in the work by Quadling on which I hadn't a notion, it became obvious that matters have progressed too far to continue the plan of coauthorship. I am not particularly distressed by residual differences in outlook—like yourself, I am sure they could be quickly resolved if we had the chance of intimate conversation, which I greatly miss. The final difficulty is that we have a different outlook on style, and I suspect that almost anything that we could compromise on under these awkward circumstances (I mean only distance) would leave us both unhappy. With some relief then I went ahead last week on a different basis, namely to write a separate paper intended to accompany your own wherever we agree to publish. So with some well considered apologies for having kept you so long, I am asking you to consider this as an alternative procedure. My own preference for journal on this basis is Genetics, partly because of the convenience of its being edited here (by Crow and Brink), partly because it ~~reaches~~ reached the audience that would be most interested and best prepared for the story. However, JGM would not do badly either. If you are concerned about paying for reprints, I think we could (if you wanted) arrange to have the two papers reprinted together and economize very considerably.

I still want to find an opportunity to join with you in a joint statement on the problem. Rather than MGB (though I would acquiesce) I would suggest submitting an abstract for the Genetics Society meeting this September. It would be published in that issue of Genetics, and be available for formal citation, e.g., if needed by Quadling. Would you want to draw one up? It needs to be under 250 words, unfortunately, and I am hoping that the accompanying ms. will give you a clear enough idea of what we might agree about. This has to be delivered to the Sect. by May 20, so there is enough, but not excess, time.

As you see for yourself, this draft is not a finished job. Most of the gaps in the data represent an incomplete assimilation of experiments already done, but there are some I definitely want to repeat or extend, particularly on further branching of chains after n_{13} , and on the partition of chain numbers. I have never had a harder job writing than this: the pedigrees are almost impossible to summarize except in terms of some concrete working hypothesis, a luxury I have perhaps masochistically refused. But the 11:20 split mentioned on p. 6 was quite unmistakable, and it worries me to have an isolated case. In one subclone, the branching occurred between n_5 - n_{15} (the 11's side); most of the 20 was a group of 18 that came from one cell reisolated at n_{15} , probably the latest branching in my experience. This doesn't bowl over your hypothesis (the question is perhaps what would?) since why should your gene not rarely split once, if it was not already essentially divided when the donor cell happened to be lysed.

You might have had this draft sooner but for a very welcome visit from Tracy Sonneborn. What a wonderful segregation story he has! There happens to be a fellow here (Kimura) who specializes in stochastic processes in genetics, and has worked out some mathematical models that so far give a surprisingly good fit to TMS's data. Tracy told me ~~nothing~~ of meeting you and the impression you made with your pedigrees on the trail story. I insisted he read the parent of this draft, rough as it was. At first he was not at all keen about my fussiness, thinking you had the mosy straightforward interpretation, but it turns he ~~had~~ had misunderstood something, and though you had identified the mcp with the gene along the lines A1 or B3c (pp11-12) which is probably the one notion we would now both favor the least! ~~I am a little surprised~~ Tracy is quite unhappy about the idea of any appreciable persistence of normal organelles in the absence of genes related to their formation; his own experience in Paramecium, with kappa and gullees has gone the other way. Still I am a little surprised at his current stand, since before I heard it from you, he had suggested *3 when we visited him en route to the Oak Ridge meetings last spring.

I don't know whether you will care to go along on the terminology, though chain and unicateate have worked out quite successfully (though I had an awkward moment on p. 15 line 24 "sequence" ha!). I think we should agree on such symbols as mcp and E. These will do for me as is, but we ought to consider other possibilities- greek letters? - which might be less suggestive.

Bruce- we had such a time together a year and a half ago, I can't help think about it without nostalgia and regret we can't operate more closely together; I know that Esther shared that sense of stimulation too. If we don't somehow manage to get to the UK ourselves first, and being a duo and at the university does impair our motility, is there any chance at all of your coming back to Madison and spending more than a paltry few weeks here this time? If you can manage the fare, I'm sure we can match your stipend, though what with Rockefeller and Commonwealth you could probably swing that yourself with no trouble at all. I really do feel frustrated at having to keep contact by mail, only/.

Anyhow let me know your reaction to the present scheme. My suggestion on procedure is to allow about a month to complete our mss and finish loose ends and decide where to send them. It was Tracy who suggested emphasizing p.18; we may want to exchange brickbats even more freely-- now's your chance. You never have sent me your crucial pedigrees mentioned in your draft, but that is sympathy, not criticism. If you can get the time to reply by tape, maybe you'll get your answer the same way; it's only fuss and inertia that's held that up.

Yours,

*1 at p13